same, 54° south; the difference in longitude is 30°. Both have a marine climate with prevailing west winds.

Station.	Mean temperature.	Days with rain or snow, 1882–83.	Precipitation.	Snow line.
	°C.		Mm.	Meters.
Cape Horn[7]	5.5	278	1,400	900
South Georgia[8]	1.4	301	900	600
Differences	4.1		500	300

The difference in precipitation is due, without doubt, to differences in the exposure of the observing stations, and probably as a matter of fact the precipitation is the same at corresponding altitudes at both places. From the geomorphologic point of view the islands west and south of Tierra del Fuego are comparable in all respects with the island of South Georgia. The altitude of the perpetual snow line in the Magellanic region[9] is 900 meters, while on the northeast coast of South Georgia[10] it stands at 600 meters.

Together with a difference of 4° C. in the mean temperature there is here a difference of about 300 meters in the two levels of perpetual snow.

In fact it seems to me useless to insist any longer on the example selected, since the elevation of the line of perpetual snow is not sufficiently well known, while our knowledge of the topography of South Georgia and of the islands of Tierra del Fuego is even less satisfactory.

Moreover, this example will inevitably be criticised, and without doubt those geologists holding to the theory of a moister climate will point out that the level of the line of perpetual snow corresponds with a great variety of isotherms, ranging between the isotherms of +3° in the Andes near Quito and that of —10° or —11° C. in Spitzbergen and Nova Zembla,[11] or even more. In fact, one may readily accuse me of partiality and claim that the above example has been chosen with the direct purpose of showing that the climate of the glacial epoch must have been much more rigorous than we have presumed. It will certainly be maintained that only a general discussion of all the known facts can have decisive value, and that in any case we should take the mean of all the numbers obtained. I do not contest this, but nevertheless in the actual state of our knowledge it is preferable to limit ourselves to the selection of illustrations from regions where the climate is now and has been essentially marine, choosing by preference oceanic islands. Therefore the example given seems to me well chosen.

Another good example deserving of thoro study and discussion is the comparison of the region about the Straits of Tierra del Fuego with the antarctic continent lying south of Cape Horn. Both regions are mountainous and exposed to oceanic winds, which bring abundant precipitation; but the altitude of the snow-line in the polar lands is lower by about 800 meters, and the actual appearance of this land is very probably that which the region about the Straits of Tierra del Fuego must have presented at the time of maximum extension of Pleistocene glaciation. What, now, are the mean temperatures of these two regions? We have but few available data for the Straits of Tierra del Fuego and still less for the antarctic lands. The known means are as follows:

Magellanic region.	°C.	*Antarctic region.*	°C.
Punta Arenas[12]	6 7	Snow Hill[16]	—11.8
Ushuwaia[13]	6.5	Scotia Bay[17]	— 5.4
Cape Horn[14]	5.5	Wandel Island[18]	— 5.4
Staten Island[15]	6.3		

We thus have a difference of level of the snow line of 800 to 900 meters, corresponding to a difference in the mean temperature of at least 10° to 12° C.

If a more thoro study of the topography and the meteorological conditions of the two regions permits us to maintain this analogy, the data already secured will not fail to clear up a part of the problem of the climate of the glacial epoch. We may at least determine how many degrees fall in temperature must occur in the Magellanic region in order that the ice again descend to the level that it then occupied. I would emphatically declare that when one speaks of the lowering of temperature that accompanied the glacial epoch in the Magellanic region and sets the same at 4° C. one speaks as tho this number applied indifferently to all the regions of the globe.

Now it is inconceivable that inland ice caps such as existed in northern Europe and North America, should not have profoundly modified the meteorological regimen of neighboring regions and even the general atmospheric circulation of the whole Northern Hemisphere. The distribution of climates, even in the regions beyond the ice-invaded country, must have been incontestably at variance with the present, so that for this reason alone it seems to be inadmissable to assume that the fall in temperature went on in a similar manner everywhere, and that it may be exprest by a simple difference—and that the same difference—from the mean annual temperature.

A PLEA FOR TERRESTRIAL AND COSMICAL PHYSICS.[1]

By Dr. L. A. BAUER, Carnegie Institution of Washington. Dated December, 1908.

Once upon a time, at a certain small dinner party, the Duke of Wellington on being urged to express his opinion frankly of the French marshals he had so successfully worsted in battle, pointed out their good qualities in a most free and magnanimous manner, showing wherein each particularly excelled. Whereupon one of the party said, "Well, sir, how was it that with such various great qualifications you licked them all, one after another?" The Duke, taken a-back, paused, then said, "Well, I don't know exactly how it was, but I think if any unexpected circumstances occurred in the midst of a battle which deranged its whole plan, I could perhaps organize another plan more quickly than most of them."

This quality of mind, to instantly change an established train of thought or to be receptive to a new set of circumstances and facts, and thus to be capable of immediately setting up a fresh plan of action, was tersely and most suggestively exprest by Maxwell when writing Herbert Spencer about a subject of controversy in the latter's "First Principles:"

> It is seldom that any man who tries to form a system can prevent the system from forming around him and closing him in before he is forty. Hence the wisdom of putting in some ingredient to prevent crystallization and keep the system in a colloidal condition.

At the Ithaca meeting of the Association two years ago last summer, I prefaced a paper on the San Francisco earthquake by a few remarks calling attention to the disparity of papers pertaining to the physics of the earth and of the universe presented to-day before Sections A and B. I stated it was

[7] Lephay: Mission scientifique du Cape Horn. Vol. II, p. 138.
[8] Die Internationale Polarforschung, 1882–3. Die Beobachtungs Ergebnisse der Deutschen Station. Vol. II, p. 138.
[9] According to the officers of the *Beagle*, 1,000 meters; according to Pissis, 800 meters; according to Thomas Bridges, 900 to 1,000 meters.
[10] Hann: Klimatologie. 1897. Vol. III p. 467.
[11] Hann: op. cit., I, p. 313. See Hann-Ward, 1903, p. 321–322.
[12] Arctowsky. Ciel et Terre, 16 juin 1900.
[13] Lephay: Mission scientifique de Cap Horn. Tome II, p. 138.
[14] Lephay: ibid., p. 271.
[15] Arctowsky. Ciel et Terre, 1 decembre 1900.
[16] Bodman. Petermann's Geogr. Mittheilungen, 1904, Hft. 5.
[17] Mossman, Scottish Geog. Mag., August 1905.
[18] J. J. Rey in Charcot: Les Français au Pole Sud. p. 367.
[1] Presented at the Baltimore meeting (1908–9) of the American Association for the Advancement of Science, before the General Interest meeting of Section B (Physics).

my impression that this had not always been the case. Attend any similar meeting abroad, be it in England, Germany, or France, and you find the names of foremost physicists down for papers on results of research in terrestrial or cosmical physics. These eminent investigators evidently find food for exhilarating thought and stimulating work in the unraveling of the phenomena of seismology, meteorology, geodesy, hydrology, atmospheric electricity, solar physics, terrestrial magnetism, etc. They appear to regard knowledge gained in the laboratory and in the university merely as a means to an end, not an end in themselves.

The chairman of the section of mathematics and physics at the recent meeting of the British Association was the well-known physicist-meteorologist, Dr. W. N. Shaw, director of the London Meteorological Office. Besides making a most suggestive presidential address, he led an interesting discussion on "The isothermal layer of the atmosphere," a live topic in meteorology to-day. Those taking part in the discussion were: Shaw, Rotch, Dines, Cave, Turner, Thomson, and Walker. Several times has it occurred within recent years at that association that owing to the number of titles presented it was necessary to have a subsection on "Cosmical Physics," which I am very glad to note did not apparently meet with the favor of the physicists themselves. Our British colleagues want the physicists to stay with them and not flock off by themselves, and the present tendency seems, accordingly, to be at the British Association not to form such a subsection. You will find among the past contributors to papers and discussions on this subject such names as, for example, Kelvin, Rücker, Schuster, Lockyer, Eliot, Cortie, Teisserenc de Bort, Glazebrook, Chree, Thomson, and others.

Doctor Shaw[2] well said that, "for the advancement of science in this sense we require all three: the professor, with academic freedom to illuminate with his genius any phenomenon which he may be pleased to investigate; the administrator, face to face with the practical problems in which science can help; and the living voice, which can tune itself in harmony with the advances of science and in sympathy with the needs of the people whom it serves."

I can not better illustrate this mutual help which may spring from friendly conference between the pure physicist and the "world-inoculated" one than to quote you a paragraph or two from a most admirable presidential address delivered by Dr. S. Weir Mitchell at the second meeting of the Congress of American Physicians and Surgeons, held at Washington in 1891, entitled: "The early history of instrumental precision in medicine." Referring to this congress of the eminent of the land in medicine and surgery, Dr. Mitchell says:

> It is here, therefore, that the open-minded may feel the broadening influence of intellectual contacts with those who have other limitations than his own; for, indeed, in our divergent attention to special studies we run some risk that, contrary to Saint Paul, the eye may say to the hand, "I have no need of thee," or the head to the body, "I have no need of thee," for as to us, also, "there should be no schism in the body." * * *
>
> What the specialist learns, until it is commonplace, is not easily enough assimilated by the mass of practitioners. At last, however, comes a time when it is, and then that whole body of medicine feels the gain in nutrition and repays the debt. The masters of our still most imperfect art, medical optics, may wisely remember that it was physicians who most distinctively recognized and diffused the knowledge that headaches and some other brain disorders are due to eye strain, and thus, while lessening our own futile labors, crowded the waiting room of the opthalmologist. * * *
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> As I have mentioned the need for continuous individual cultivation of our multifarious science on a broad scale, and for personal consultation, I like to enlarge the plea and call a meeting like ours a general consultation. And this, in fact, it is; a focal point for condensed opinions, for authoritative statements, for criticism from varied standpoints, and for significant indications as to those accepted gains which ought to become from time to time a part of the mental equipment of all other special and, indeed, of all general practitioners.

[2] See Monthly Weather Review, December, 1908, 36:414.

Change the words physician, surgeon, medicine, to corresponding ones applicable to this gathering, and what apter or truer characterization of what our own aims and purposes should be, could be given than is embodied in these words. One is tempted to wish that we might also, like the "Deutscher Naturforscher und Aerzte Versammlung," of Germany, gather with us in annual conclave, the physicians and surgeons as well. Picture to yourselves the opportunities this would afford for enlivening and quickening discussions in several of our sections and you will appreciate what I am seeking to emphasize especially here, with regard to open, general meetings between the generalists and the "broadened specialists."

I say "broadened specialists" advisedly, for I believe upon critical examination it will frequently appear that the very pursuit of a specialty has a widening influence not adequately appreciated by one whose sphere of activity is restricted solely within the bounds of his own general science. For there is no more patent and suggestive fact of present-day research than that the most rapid achievements are not in the older, well-recognized sciences, but in their borderlands or "twilight zones." Thus the true research worker soon finds it necessary to make excursions into regions beyond what he had been regarding as his own particular zone. He makes new acquaintances, learns new customs and laws and gradually begins to preceive that there really is no well-defined line of demarcation, like the famous Great Wall of China, between one science and another.

One of the recent fundamental researches on the motion of the moon has been made by a college professor who tho an American resident got his chief training and inspiration at Cambridge, England. This same investigator has contributed articles on meteorological mechanics. Columbia University, in its admirable endeavor to present a popular course of lectures on subjects of applied physical science, must draw for its lecture on "Atmospheric Phenomena and Physical Theory," upon another foreign born, Cambridge-inspired, now American resident physicist. There are a number of you whose work lends additional eloquent testimony to the broadening and cosmical influence of that eminent school of physics. However, there are other European departments of physics of which much along similar lines could be stated and exemplified. Is it not possible to have more home-inspired university product to draw upon in these fields? Couldn't our country be more adequately represented on international comittees formed to consider and investigate some of the great world-wide questions? I do not believe we lack the talent. If there is less incentive among us, why is it?

The fact I wish to emphasize is strikingly shown by glancing for a moment at the general character of the papers presented before the section on general physical science in the first two decades of the history of the association. The papers classified under physics of the globe, meteorology, geodesy, and navigation, frequently exceeded those in physics, chemistry, mathematics, and astronomy, whereas now, as you all know, they are in a mimority. Among the authors of the first-named papers we find names which as soon as heard you will identify as among the most distinguished of the college professors of the middle of the last century: Redfield, Bache, Olmsted, Coffin, Alexander, Henry, Silliman, Peirce, Loomis, Espy, Horsford, Guyot, Lovering, Dana, Trowbridge, Mitchell, etc. Among the more eminent of those occupying government positions we find again Henry and Bache, and such men as Maury, Davis, Hunt, Hilgard, Schott, etc. The mental grasp of many of these geophysicists and cosmical physicists was considered sufficiently broad to make them desirable timber for the highest positions of honor in the Association.

In those "good old days" some of the best contributions in meteorology and terrestrial magnetism were made by the college professor. Bache made a magnetic survey of Penn-

sylvania early in the forties, while still a professor at Girard College, where he also established the first magnetic and meteorological observatory in this country; John Locke, the inventor of the electro-chronograph (which by the way is unique in the history of science in this country as being the only scientific invention, I believe, receiving an award from our Congress, viz, $10,000), in the thirties and forties undertook a magnetic survey of North America with Cincinnati as a base station. He even extended his investigations into Canadian territory and made many of the early observations of the three magnetic elements in the Eastern States. Locke was a contemporary of the astronomer Mitchel, holding the chair of professor of chemistry (inclusive of physics) and pharmacy at the Ohio Medical College. He lived at the time when the college professor frequently had to acquire his instruments of research and pay the expenses of his experiments out of his own meager salary. Yet he found ways of doing it and, moreover, seemingly had the necessary time to go beyond his classroom and extend his good work in the territory round about and far away.

Loomis's work on the aurora borealis is still quoted. The contributions to meteorology by Espy, Redfield, Coffin, Maury, and Loomis are known even to those of us who do not profess to be meteorologists. These few illustrations must suffice for our present purpose.

If the American college professor lacks the necessary time and incentive during the scholastic year, why doesn't he do as Bache, Loomis, and Nipher did, who spent their vacations in the open in order to learn something of the physical laws governing mutual phenomena?

Why is it that in spite of the truly wonderful spirit of research that has literally seized us in this country there are so few to be enrolled among those who are making definite contributions to terrestrial and cosmical physics? We find the American physicist very prominently represented, indeed, in astromony and astrophysics. May we not hope that he will soon realize that this planet on which we dwell and which must form the basis of all our astronomical speculations is also worthy of the highest and most unselfish devotion? That, indeed, to reap the full and most lasting benefit of our celestial researches we must keep equal pace with our terrestrial ones! Will he not recall that nearly every one of the great physicists he is so justly proud of citing has at one time or another extended his mental vision beyond the problems immediately before him and considered what the application of his laboratory discoveries might be toward solving some of the riddles of the universe, or how he might benefit mankind? Faraday, Maxwell, Kelvin, von Helmholtz, Hertz, Mascart, Langley, and Rowland are but a few of the inspiring names.

Happily, there are already some indications of a reawakening and we note with pleasure the example recently set by the retiring president of the Association (Prof. E. L. Nichols), who turned his sabbatical year to fruitful use in the study of some perplexing atmospheric phenomena, and whose retiring address was largely devoted to terrestrial and cosmical physics. We note movements at some of our large universities to expand their graduate courses in the direction of terrestrial and cosmical physics. There were twelve papers before sections A and B on the subjects under discussion.

Von Helmholtz, as many of you know, from actual experience, was a notoriously poor lecturer. He seemed utterly incapable of imparting his vast knowledge in any systematic manner, and doubtless the chief value which his listeners got was the inspiration imparted by class room association with this gifted man. Von Bezold, who delivered the Berlin memorial address on von Helmholtz, told me the latter gave as the reason of his inability to impart his acquired knowledge methodically, was because he, himself, had not gained it in that way. He would take up his mathematics, for example, only when he required it—not by going systematically and consistently thru a volume of higher analysis without some impelling or suggesting motive. And so it was with the other sciences with which he had to familiarize himself in order to push to successful completion an intricate and complex piece of research. Yet how truly marvelous was the grasp this man displayed in so many varied subjects!

Now who that has ever attempted to apply his knowledge to fields outside his own immediate one has not felt this same irresistible, impelling, burning desire to know all that had been done before him in the new country he is about to explore? Have not we each one of us found that with such an all-conquering impetus back of us, the most complex mathematics or the most abstruse subject teems with a new and living interest? What was irksome before has now become a pleasure. And if there is one of you who for lack of excursions into such green pastures, has not had new and invigorating blood course thru his veins and has not been given a glimpse of a higher, truer, and more ennobling vision of life, he has missed the greatest pleasure and the highest compensation open to the research worker!

Do you know of a school of thought that has prevailed for any length of time without resisting that most subtle and therefore most dangerous of all insidious modes of attack, viz, the one coming from within its own fold of devotees, due to the pernicious habit of in-breeding? Is there any greater danger than that which besets a university that fills its chairs repeatedly from among its own graduates? We all know of the fallacy of the brilliant professor who thinks his ideas can be made to continue longest by surrounding himself with assistants drawn, if not entirely, at least chiefly, from among his own disciples. Will he not surely find, as Maxwell put it, that his "system has closed him in before he is forty" because he has forgotten the element essential to prevent crystallization—the importation of fresh blood and the introduction of new ideas?

If you agree with the speaker thus far, may it not happen that precisely similar occurrences be recorded of our societies, because of the suicidal policy of a particular class of members who are apt to believe that the best result can be reached by increasing their representation, and thus by their majority vote be able to dictate and control the general policy of the society to which they belong? Is it a wise organization for membership in any deliberative scientific body to be so constituted as to make it possible for the act of the assembly to be unduly influenced by one set of investigators? Is there not here subject for careful thought—a source of degeneracy due to in-breeding in societies to be equally guarded against? Joseph Henry truly said: "Votes in science should not be counted, but weighed."

This, then, is my specific plea: A broader conception and a more scientific representation of the subjects of physical research. Could we not make the attempt certainly once a year to devote most of our time and attention to some of the greater aspects of our work and take stock, so to speak, of our achievements, and of their possible applications?

RETIREMENT OF PROFESSOR KLOSSOVSKII.

By Prof. ALEXANDER ZIWET, Ann Arbor, Mich. Dated February 6, 1909.

A. Klossovskii.—The last page of the journals "Meteorological Review" (Publications of the meteorological net of southwestern Russia, 1887–1908) and "Annals" of the magnetic and meteorological observatory of the Novorussian University [at Odessa], 1894–1908. Odessa, 1908. 8vo. VI. 84, 244, 104 pp., 2 plates.[1]

[1] Page finale des journeaux "Revue météorologique" (Travaux du réseau météorologique du sud-ouest de la Russie, 1887–1908) et "Annales" de l'observatoire météorologique et magnétique de l'université impériale à Odessa, fondés par A. Klossovsky. Odessa. 1908. v p. 8°. (Russian text.)